One would like to see "piles of shot" relegated to the examples, as in these days of rifled guns and elongated projectiles it seems an anachronism. The book is logical well printed, and illustrated by the best set of examples that can be found in any book of the same kind.

P. A. MACMAHON.

OUR BOOK SHELF.

My Telescope. By a Quekett Club Man. (London: Roper and Drowley, 1888.)

THIS volume is described by its author as a simple introduction to the glories of the heavens. It is not designed as a guide to the use of a telescope, but simply to give such an account of its teachings as may interest non-astronomical readers. The main features of the various celestial bodies are described, but, for some reason or other, comets are not considered at all. Most of the descriptions are very meagre; thus, nebulæ and star-clusters are disposed of in a page, and that not closely printed; even the sun—"the ruler of our system"—is described in a little over three pages. The scantiness of the information given is the greatest fault of the book.

In the little that the book does contain, many mistakes occur. Thus, the moon is stated to present a marbled or mottled appearance because her surface is unequally refractive (p. 62), and the velocity of light is twice put down as 184,000 miles per second (pp. 46 and 72.)

The illustrations are moderate, and the book has a generally neat appearance. The place it is to occupy in astronomical literature, however, is not very clear, as there are already many cheaper books in existence which contain the same information, and much in addition.

Hand-book of Perspective. By Henry A. James, B.A. Cantab. (London: Chapman and Hall, 1888.)

THIS small book contains the principles of perspective explained in a plain and concise way. The author seems to have taken great trouble to make his meaning as clear as possible, and has spared no pains in getting together a good collection of examples, which are all worked out and accompanied in each case by a diagram.

The examples themselves would form a useful and practical course on the subject, since they are arranged in a progressive order, starting with the projection of a single point, and taking up in turn lines, surfaces, and solids.

Beginners will find this volume very serviceable to them, pictures as well as diagrams being given to illustrate the various positions of planes, lines, &c.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Coral Formations.

IN the last paragraph of my letter which appeared in your issue of the 15th inst. (p. 462), I remarked:—"It is quite reasonable to suppose that the dead coral so dissolved in the formation of lagoons is carried out as material for fresh coral growths."

Mr. T. Mellard Reade, in a letter on the same subject, dated 22nd inst. (p. 488), in criticizing the results published by Mr. Ross's letter of the 15th (p. 462), remarks:—"I believe that at no place on the surface of the globe are such dead shells being supplied at a rate that would even balance this supposed rate of chemical destruction."

Can Mr. Reade give any observations or figures in support of his view of the rate of accumulation of oceanic calcareous deposits?

Laying aside all question as to arithmetical error, and without committing myself to the accuracy of Mr. Ross's figures (or even insisting on my own), as to the amount of dead carbonate of lime dissolved in any given time by sea-water in lagoon formation, but taking it as a fact that it is soluble in a marked degree (as is proved by the experiments made by Mr. Ross and myself), and that coral reefs can only exist in regions under the influence of the great warm tropical ocean currents, then we may expect the waters of coral-bearing regions to contain a greater proportion of lime than is found elsewhere, thus forming the calcareous food for continuous extension of the coral formations.

Of course, a distinction must be drawn between the so-called *dead* and living coral, in thus far that the latter is protected from the solvent action of the sea-water by its vitality, while the former, as referred to in my last letter, is peculiarly susceptible to this influence.

ROBERT IRVINE.

Royston, Edinburgh, March 26.

Professor Rosenbusch's Work on Petrology.

READERS of NATURE interested in the study of petrology will be grateful to Dr. Hatch for his lucid review of Rosenbusch's great work, and those who are not able to profit directly by the German original will be glad of the *résumé* given of the latest classification of the massive rocks according to the views of the greatest living authority on the subject.

A translation of Rosenbusch's book into English is much to be desired. Rich as we are in fragmentary literature on the subject, a leading text-book is still wanting. Dr. Hatch would deserve well of his fellow petrologists if he would give them a translation of the work he reviews so well. There might be room for some cutting down in dimensions, especially in the treatment of the "neo-volcanic" rocks. Rosenbusch himself is conscious that this part of the work is, perhaps, a little overloaded with detail, as he says that with a new structure "the scaffolding is not removed before the house is finished," and possibly a competent translator might consider that a little less scaffolding would still be sufficient.

There will no doubt be more or less difference of opinion among authorities as to the correctness of the views which have governed Rosenbusch in his system of rock-classification, especially on one or two points. But none will deny that this classification, with the immense research and study accompanying and supporting it, fully given to the student in this latest work, are a splendid achievement.

Dr. Hatch does well, especially in the interest of younger workers in petrology, to insist on the purely arbitrary nature of *any* system of classification, so far as the separate "rock-types" are concerned; such types passing more or less gradually into others, on either side of them, in all cases. Rosenbusch himself points this out, but a further emphasis of the warning was well in place.

In working with a large text-book like the one in question, with its minute treatment of small details, the student is apt to neglect this consideration of the passage of one rock into another, or at any rate to devote too little attention to it. Nothing, however, could be a greater safeguard to him in this respect than to make for himself a tabular arrangement of Rosenbusch's system, so that the eye can follow in a moment the relationships of the different rock-types to each other. I think it is a pity that such a map, as it were, does not accompany the book. The attentive study of it would not only much assist the worker in his detailed use of the book, but would also greatly aid the beginner to "keep his head level" and steer clear of the sad pitfall of going in too much for "pigeon-holes."

Perhaps an outline table of this sort, which I inclose, may be of a little use to some of your readers, if only to give a compact view of Rosenbusch's classification and of how he connects the main rock-types in series through the four divisions of plutonic rocks, dyke-rocks, palæo-volcanic rocks, and neo-volcanic rock.

An amplified table on the same model, with the various

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leading sub-types under each main type, is what I have tried to indicate. Such a table makes clear at once not only the passage of, say, syenites into trachytes, diorites into andesites, but also that of trachytes into andesites, andesites into basalts, &c., &c.

Effusive Rocks		Dyke Rocks	Plutonic Rocks
Neo-volcanic	Palæo-volcanic		
Liparites Pantellerites	Quartz-Porphyries	Muscovite-Granite (Aplite) Granite-Porphyries	Granites
Trachytes Basic Pantellerites	Quartzless-Porphyries	Syenite-Porphyries Syenitic Lamprophyres	Syenites
Phonolites Leucitophyres		Elæolite-Porphyries	Elæolite Syenites
Dacites Andesites	Porphyrites	Diorite-Porphyrites Dioritic Lamprophyres	Diorites
Basalts	Augite-Porphyrites and Melaphyres		Gabbros and Norites
			Diabases
Tephrites Basanites			Theralites
Limburgites Augitites	Pikrite-Porphyrites		Peridotites
Leucite Rocks			
Nepheline Rocks			
Melilite Rocks			

A student who has attempted, with plenty of good sections to work on, to draw a definite line between trachytes and andesites, or augite-andesites and basalts, will probably not easily again fall into the error of believing in hard-and-fast types!

Dr. Hatch has at once indicated two main points which will strike petrologists as open to criticism in Rosenbusch's system: the "dyke rocks," and the subdivision of the effusive rocks into "palæo-volcanic" and "neo-volcanic." With regard to the former it deserves to be pointed out that it has not been found possible to classify any representatives of the diabases and gabbros under the head of dyke rocks at all; and one feels that very considerable force has been used, in some other cases, in order to get the rocks under this division into their proper "pigeon-holes."

Something of the same sort of strain and artificiality is felt in some cases with regard to the two divisions of the effusive rocks, and here again it is, perhaps, the equivalents of the diabases and gabbros which most strongly exemplify this feeling—viz. the augite-porphyrites and melaphyres, and the basalts.

Many passages, in several parts of the book, show how fully conscious Rosenbusch is of the weak places there are in his system, as there must always be weak places in every system in a young and rapidly growing science like microscopic petrology. One does not know which most to admire—the wonderfully wide research and knowledge, and the skill and painstaking care and labour, with which the system of classification has been evolved out of the great amount of material to hand, or the great modesty with which it is presented to us.

It will be a thousand pities if a translation of the book does not before very long find its way into the hands of all English-speaking petrologists. A. B.

Manchester, March 19.

"The Mechanics of Machinery."

ANY errors—or what seem such—in a work of so high character and repute as Prof. Kennedy's "Mechanics of Machinery" may be conveniently noticed in NATURE; and therefore no further reason need be offered for the following remarks.

(1) "If, then, a body is moving with a linear velocity of v feet per second about a centre (permanent or virtual) at radius r feet, it is undergoing a radial acceleration of $\frac{v^2}{r}$ foot-seconds per second, and the centrifugal force corresponding to this acceleration will be $\frac{v^2}{r}$ pounds per unit of mass . . . ." (p. 228)

Surely there is some confusion here between the virtual or instantaneous radius, and the radius of curvature; for it is of course the latter which indicates the radial acceleration.

It has always seemed to me that the common expression that a body is turning "about," or, as Prof. Kennedy sometimes puts it, "round," its virtual centre is very apt to mislead. All that we are entitled to say is that at any given instant one point is at rest; and that all other points are moving in directions at right angles to the lines joining them with that point, and with velocities proportional to those lines. This being true also in the simple case of motion in a circle we are apt to use the same language to express it, with the inevitable suggestion of the *curvature* being in correspondence. It need not be said that the curvature may be zero (as when a circle rolls inside one of twice its diameter); or that it may be *away from* the centre (as when the former circle is more than twice the diameter of the second). The beginner would find it hard to realize the latter case when he has been taught to speak of the body "turning round" its virtual centre. I do not know that any better description can be adopted, but it ought to be introduced with emphatic cautions that it is merely a convenient device of description. In any case it seems to me that the old term "instantaneous centre" is more likely to keep the truth before us than the "virtual centre" preferred by Prof. Kennedy.

(2) In a series of interesting discussions about train resistance, the following passage occurs: "The brake resistance is 1200 pounds, but it has to be overcome through a distance π times as great as that moved through by the train as a whole . . . . ;" this brake resistance having been described above as "the frictional resistance at the periphery of each wheel."

I cannot follow this. Surely if the circumference of the wheel be x feet, by the time the train "as a whole" has run over a rail-length of x feet, the brake has slid over a wheel-circumference of x feet also.[1]

This need hardly have been noticed, but that the examples seem always very carefully chosen, not as fancy problems, but as being in accordance with practical experience. Either, therefore, the common impressions as to the time and distance requisite for stopping must be wide of the mark, or the value assumed, in this and a number of other examples, for the brake resistance, must be about three times too small.

(3) "If, for instance, the brakes had not acted promptly, and had been put very hard on at the end, the velocity and acceleration curves might have been as dotted, when the maximum acceleration occurs almost at the end, a state of affairs very uncomfortable for the passengers" (p. 205).[2]

Is this so certain? I mean, is there any reason to suppose that a sudden change of *acceleration*, not of *velocity*—for of this there is no question with any conceivable brake system—would be felt as disagreeable?

The question must, one would think, have often presented itself to speculative and scientific engineers; but as I do not remember to have seen it discussed one would like to get their opinion on the point. The way it strikes me is this. We cannot practically suppose the acceleration *put on* instantaneously, for the brake needs time to work; but we can get as near as Nature allows to the instantaneous when it is *taken off*. That is, if the brakes are left on to the end, the velocity continues to diminish

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[1] In the *Errata* we are told to omit this passage; but as the premises and the conclusion, between which it formed the necessary connection, are left unaltered, the need for it is as great as before. How else, but by such an inference, can the conclusion be reached?

[2] The curve of velocity here is a parabola, or nearly so, representing (on a distance scale) a nearly constant "acceleration," with a constantly decreasing velocity.

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